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The Debate on Right-to-Carry Concealed Weapons Laws

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Working Paper Number 71

February 2008

Moody thanks the School of Economics at the University of Adelaide for research support during a semester visit.

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DEPARTMENT OF ECONOMICS
WORKING PAPER # 71
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Abstract

There are a large number of studies indicating that "shall-issue" laws reduce crime. Only one study, by Ayres and Donohue, implies that these laws lead to an overall increase in crime. We apply an improved version of the Ayres and Donohue methodology to a more complete data set. We find that Ayres and Donohue's results, projected beyond five years, and our own analysis imply that shall-issue laws decrease crime and the costs of crime and are therefore socially beneficial.

JEL Codes: K14

Keywords: Crime, gun control, concealed carry laws

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I. INTRODUCTION

In 1997 John Lott and David Mustard published, "Crime, Deterrence and Right-to-Carry Concealed Handguns." They studied "shall issue" right-to-carry concealed weapons laws that require authorities to issue concealed weapons permits unless the applicant was not qualified because of a criminal record or mental instability. They found that states with such laws had lower violent crime rates, presumably because these laws would result in more people carrying concealed weapons. Criminals might be less willing to engage in violence if they couldn't tell which of their prospective victims were armed.[1] The article created a furor and the debate continues. In this paper, we review the main threads of the discussion in the literature and attempt to extend the debate with our own statistical analyses.[2]

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[1] In states with shall issue laws, authorities are required to issue right-to-carry concealed weapons permits to anyone who applies, unless the applicant has a criminal record or a history of mental illness. States with "may issue" laws allow a considerable amount of discretion on the part of the issuing authorities. Typically, such states require that the applicant demonstrate a particular need for a concealed weapons permit. Self-defense is usually not a sufficient reason to be granted a permit in may-issue states. Consequently, shall-issue states can be expected to have more citizens with concealed weapons permits, and presumably more people carrying concealed weapons.

[2] Much of this debate takes place in op-ed columns, letters to editors, internet chat rooms, and web logs. In this article we concentrate on the academic debate.

II. CHRONOLOGY

The original study by Lott and Mustard (1997) used pooled time-series and cross-section data across all the counties in the United States for the years 1977 to 1992. They used the fixed-effects panel data model, which corrects for possible unobserved heterogeneity across counties. They included time dummies, arrest rates, several income variables and a host of detailed demographic control variables. The target variable was a dummy variable that took the unit value for those counties in shall-issue states during or after the first full year of implementation, zero otherwise.[3]

The primary set of results was reported in Lott and Mustard's Table 3 (1997, 20-23). The estimated coefficient on the shall-issue dummy variable was negative and significant for all the violent crimes (murder, rape, robbery, and assault), positive and significant for larceny and auto theft, and not significant for burglary. The estimated coefficients were also large enough numerically to cause substantial reductions in the estimated costs of crime. Lott and Mustard then followed up this analysis with a corresponding state-level model (1997, 27). They found that all violent crime categories were significantly reduced by shall-issue laws, again with large implied reductions in the costs of crime. They then engaged in a series of robustness tests all of which confirmed the basic finding that right-to-carry laws reduced violent crime.

Contrary findings appeared very quickly. Black and Nagin (1998) noted that Lott and Mustard, by using a single dummy variable for the shall-issue law, assumed the same effect for all states and all years. They extended the model to allow for separate dummies for each state and found that the results differed across states with some states significantly positive, some significantly negative, and some showing no effect. They also estimated a first-differenced model using pre- and post-law dummy variables for the five years before and after the adoption of the shall-issue law. Finally, they estimated a

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[3] Lott and Mustard also tried a shall-issue variable that took a fractional value indicating the proportion of the year the law was in effect in its first year, the results were unchanged.

model with individual state trends as additional controls (but with a single shall-issue dummy). They concluded that the Lott and Mustard results were fragile and that, overall, the shall-issue law had no significant effect on crime.

In the same issue Lott (1998) responded to Black and Nagin. He pointed out that they ignored the Lott and Mustard (1997) models where the shall-issue law dummy was interacted with population, used linear and quadratic trends, and used the number of permits. Thus their criticism that the Lott and Mustard model relied on a single dummy variable was misplaced. However, the most telling criticism concerned Black and Nagin's use of pre- and post-law dummy variables. If crime rates were generally increasing prior to the passage of the law and falling after, the "inverted V," as Lott and Mustard reported (1997, 35), the coefficients on the dummy variables for the two or three years before the law could be expected to be approximately the same as the corresponding coefficients for the two or three years after, implying no effect of the law, when the law in fact had a very significant effect on the crime rate.[4]

Black and Nagin also criticized Lott and Mustard for not including individual state trends as controls for potentially omitted variables. However, as Lott pointed out, the original paper had reported first differenced models, one of which included state dummies. In such a model, the state dummies are equivalent to individual state trends. Lott also argued that the original paper had allowed differential impacts across states in the sense that individual analyses were done for Pennsylvania and Oregon where data on the number of permits were available. Despite the fact that Lott responded to each of the Black and Nagin criticisms, the issue was not resolved.

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[4] Note that this criticism of the dummy variable method only applies to short periods after the passage of the law. If the law remains in force for many years and crime falls continuously, the average effect estimated by the dummies will eventually be negative.

At this point, the broad outline of the subsequent debate was already in place. Future work on this issue would have to address the problem of differing before and after trends (including the inverted V), allow for individual state trends, and allow the law to have differing impacts across states.

Two years later, Lott (2000) extended the sample to 1994 and introduced spline models to address the inverted V problem. Lott examined many alternative versions of the model and determined that the results were very robust. Shall-issue laws were found to significantly reduce violent crime.

In 2001 the proceedings of a conference on shall-issue laws were published in the *Journal of Law and Economics*. In that volume, several studies confirmed the hypothesis that shall-issue laws reduce crime. One year later, 2002, the second edition of *More Guns Less Crime* was published. Lott extended the sample to 1996 and re-estimated the spline models, along with a host of alternative specifications. Shall-issue laws were again found to reduce violent crime.

At this point in the debate, the weight of evidence was firmly on the side of those claiming that shall-issue laws cause violent crime to fall. However, Ayres and Donohue (2003) significantly shifted the debate. They noted the possibility of selection bias in the aggregate model where early adopting states are in the data set for many years and late adopting states are barely represented. Thus, the aggregate model with a single dummy or trend for all states, when extended over many years, is eventually reflecting only a few states, not the entire country. For example, if the aggregate model is extended over 13 years, of the 24 states that have passed shall-issue laws since 1977, only Maine and Florida have had a shall-issue law for that long and may not be a representative sample for the country as a whole.

Ayres and Donohue also claimed that the original 1997 Lott and Mustard paper, which was based on data from 1977 to 1992, included only states that adopted shall-issue laws in the 1980's when crime peaked because of the crack epidemic. Thus, the fall in crime after the crack epidemic subsided was being reflected in the negative coefficients on the shall-issue dummy variables. This omitted variable criticism applies to some of the Lott and Mustard regression models. Ayres and Donohue argue that by extending the county data set to 1997, they are allowing the states that passed the law after the crack epidemic was over to help determine the effect of the law and mitigate the effect of the crack epidemic on the results. However, Lott and Mustard estimated, but did not report, a model including the price of cocaine. They found that the results were not affected. Also, the presence of time dummies should mitigate the effects of the crack epidemic unless the shall-issue states are more affected than other states. Finally, in his book Lott had extended the sample to 1996 and included states passing laws after the crack epidemic subsided, with no change in the general conclusions. The single additional year added by Ayres and Donohue is unlikely to have a significant effect. Nevertheless, the Ayres and Donohue criticism points to the need to control for the effect of the crack epidemic.

Ayres and Donohue (2003) estimated a model with individual state trends, individual state post-law dummies, and individual state post-law trends. This model, dubbed the "hybrid" model, is a generalization of the Lott spline model. The spline model assumes that the before and after trends look like a V or inverted V, thereby disallowing an immediate impact of the law. The hybrid model allows for both an immediate effect and a post-law trend. Ayres and Donohue concluded, using the hybrid model that, "For every crime type, there are more states where shall-issue laws produce a positive and statistically significant coefficient than states that produce a negative and statistically significant coefficient." (1232.) They also computed the net effect of the law across all states. They estimated, " … an increased cost ranging between $3 and $524 million." (1284) Thus, Ayres and Donohue present evidence that shall-issue laws increase crime.

However, Ayres and Donohue limit their analyses to the first five years after passage of the law. This has the effect of emphasizing the impact of the dummy variable and downplaying the impact of the long-run post-law trend. Since they find that effect of the shall-issue law on crime is generally positive in the short run but generally negative in the long run, this directly affects the overall result. We can show this by calculating the implied short and long run benefits and costs using Ayres and Donohue's estimated coefficients.[5] The cumulative effect of the law is computed by combining the estimated coefficient on the dummy variables with the corresponding coefficient on the trend variables using the formula,

$$E_i = N\hat{\beta}_{1i} + \left(\sum_{t=1}^{N} t\right)\hat{\beta}_{2i}$$

where E_i is the effect for state i, N is the number of years the law has been in effect, $\hat{\beta}_{1i}$ is the coefficient on the shall-issue dummy for state i, and $\hat{\beta}_{2i}$ is the coefficient on the post-law trend for the same state. Their results imply an immediate increase of $4.23 billion in crime costs from the dummies, with an accompanying decrease of $1.25 billion per year from the trends. Thus, after 5.77 years, the long run benefits exceed the short run costs and the benefits continue to grow continuously. Ayres and Donohue stop their calculations at five years, ignoring the $1.25 billion per year reduction in crime costs in all further years.[6]

The Ayres and Donohue article was followed in the same issue by a response by Plassmann and Whitley (2003) and a rejoinder by Ayres and Donohue (2003a). Plassmann and Whitley responded that counting states with positive versus negative coefficients is not enough. Using Ayres and Donohue's own estimates from the aggregate model, they show that crime declines after shall-issue laws are passed. In their rejoinder, Ayres and Donohue note that the results of the aggregate model that Plassmann and Whitley used were only presented to show how wrong one can be when combining effects across states.

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[5] Their coefficients are taken from Ayres and Donohue (2003, 1310-1311). They are also available on Ayres' website, http://islandia.law.yale.edu/ayers/indexempirical.htm.

[6] The breakeven formula is derived in the Appendix, although the results can also be derived by simple arithmetic.

Further, their F-tests rejected the null hypothesis that the effect of the laws was the same across states, rejecting the aggregate model.

In 2004, the National Research Council of the National Academies produced a meta-study on gun violence that concluded with respect to shall-issue laws that, "… with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates." (National Research Council 2004, 150) However, the Committee did some independent analyses that indicated that shall-issue laws reduce murder. (National Research Council 2004, 269-70)

The debate is summarized in Table 1. The weight of evidence indicates that shall-issue laws reduce crime. If we rely only on studies appearing in peer-reviewed journals, we would have to conclude that shall-issue laws reduce crime. Although Ayres and Donohue conclude that, "… the best evidence suggests overall small increases in crime associated with the adoption of shall-issue laws," (2003, 1397), this conclusion relies on ignoring the long run reduction in crime as a result of these laws.

Therefore, at this stage in the debate, the weight of evidence appears to support the crime reducing property of shall-issue laws. In the next section we apply an improved version of the Ayres and Donohue model to the county level data set extended to 2000 to see if the Ayres and Donohue results continue to hold.

Table 1 about here.

III. SHALL-ISSUE LAWS REVISITED

We apply the Ayres and Donohue hybrid model to the county data set extended through 2000, which encompasses all existing law enactments and three additional years of data.[7] We also modify their model by adding two new variables. Ayres and Donohue argue that Lott and Mustard, by relying on data from 1977 to 1992, create a spurious correlation between the adoption of right-to-carry laws and crime

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[7] The dataset is available at http://www.johnlott.org. This is the data set used by all the studies cited above, extended to the year 2000.

due to the effect of the crack epidemic. This is an omitted variable problem. Fortunately, a measure of crack cocaine has been recently developed by Fryer, *et. al.* (2005) for the purpose of addressing this problem which also plagues other crime studies. The measure is derived from cocaine arrests, cocaine-related emergency room visits, cocaine-induced drug deaths, newspaper reports, and DEA drug busts. This variable allows us to avoid any spurious correlation with the crack epidemic.

The second variable included here that is omitted from the Ayres and Donohue model is a lagged dependent variable. This variable is used to capture dynamic effects across time. An equation with a lagged dependent variable is a first-order difference equation, which can display patterns of growth, decline, or oscillation. The Ayres and Donohue model is completely static. It suffers from potentially serious omitted variable bias if the lagged dependent variable is significant. In addition to these two variables, we include all the variables used by Ayres and Donohue including individual state trends, county dummies, and year dummies. Like Ayres and Donohue we disaggregate the effect of the shall-issue law to the state level. The target variables are the individual state shall-issue dummy variables and corresponding post-law trends. The shall-issue dummies take the unit value in the first full year following the passage of a shall-issue law. The post-law trends are zero up to the year of passage with the trend starting in the first full year after passage. We use Lott's coding.[8] The sanction variables are the arrest rate for violent crime, the arrest rate for property crime, the per capita prison population, and, in the case of murder, the execution rate.[9] The control variables are those used in previous analyses. The variable names, definitions, and means are presented in Table 2.

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[8] There is some disagreement as to the exact dates of the passage of the various shall-issue laws. In preliminary analyses we used both the Ayers and Donohue dates and the Lott dates. The results were the same. We use the Lott dates in this paper.

[9] The arrest rate is the clearance rate (arrests/crimes). The arrest rate could be endogenous in the crime equation. For that reason we use the arrest rate for all violent crimes in the murder, rape, robbery, and

Table 2 about here.

The shall-issue laws are state laws, applicable to all counties within the state.[10] Consequently, all counties within a state have the same values for the shall-issue dummy and post-law trend, implying that the errors are likely to be correlated across counties within states. This causes the usual standard errors to be underestimated and the t-ratios to be overestimated, potentially causing spurious correlation between the shall-issue laws and crime rates (Moulton 1990). To avoid this problem, we use heteroskedastic-consistent ("robust") standard errors corrected for clustering within states.[11] Because of the large number of zeroes in the murder and rape variables, 39 percent and 21 percent respectively, we add a small constant, .10, to these variables before taking logs. This changes the mean, but not the variance and therefore does not create measurement error.

The results with respect to the interesting control variables are presented in Table 3.[12]

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assault equations and the arrest rate for all property crime in the burglary, larceny, and auto theft equations. This allows us to dodge the simultaneity issue.

[10] Except for Philadelphia, which was initially exempt from Pennsylvania's shall-issue law.

[11] Ayres and Donohue did not correct their standard errors for clustering.

[12] To conserve space, we do not report the coefficients on the 36 demographic variables, the individual state trends, the year dummies, and the individual county intercepts. The coefficients on the shall-issue law shift dummy and post-law trend variables are presented in Tables 5 and 6 below. Complete results, data, and Stata programs are available at the senior author's website. We do not compute equations for total crime, violent crime, or property crime because these aggregates merely count the various subcategories. Therefore, because there are so many more assaults then there are murders, rapes, or robberies, violent crime is virtually indistinguishable from assault. Similarly, property crime and total crime are dominated by larceny, the most common type of index crime.

Table 3 about here.

The crack variable is significant and positive in all of the crime equations, except murder and rape, indicating that the crack epidemic had significant effects on most crime categories. Of the sanctioning variables, prison population has a significantly negative effect on murder, robbery, burglary, larceny, and auto theft. Arrest rates have negative and significant impacts for all crimes.[13] Real per capita income (rpcpi) is negative and significant in the rape, burglary, and larceny equations and positive in the auto theft equation. Real unemployment insurance payments (rpcui), real welfare payments (rpcim), and real pension payments are significant only in the assault equation. The poverty rate is not significant in any of the crime equations. The population level (popc) is negatively related to rates of rape, robbery, burglary, and larceny and positively related to the murder rate. The lagged dependent variable is significant in all of the equations except murder, indicating the importance of dynamic effects in most crime categories. Although we suppress the thirty-six demographic variables for readability, they are significant as groups and are therefore retained in the regressions. The year dummies and individual state trends are also jointly significant.[14]

The results with respect to the state-specific dummy variables are presented in Table 4.

Table 4 about here

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[13] The execution rate was not significant in the murder equation and was dropped. The results were unchanged.

[14] We tested for and found significant negative autocorrelation in the rape, robbery, assault, and auto theft equations. The effect of negative autocorrelation on the standard errors and t-ratios is unknown. Because we use heteroskedastic consistent standard errors corrected for clustering on states, we partially correct for autocorrelation. We believe that our hypothesis tests are valid.

Note that, for all crimes except robbery and burglary, the number of states[15] with an immediate increase in crime is larger than those with an immediate decrease upon passage of the law. Also, the population-weighted average across all states is positive for all crimes except rape and burglary and significantly positive for assault and auto theft. We computed the harm-weighted long run effect of these laws by multiplying the implied change in the number of crimes by the cost to the victims of each type of crime. The victim costs are taken from Miller, Cohen and Wiersema (1996), Table 2 and are adjusted to real 2000 dollars using the consumer price index (cpi-u-rs). The relevant costs are: murder $3.44 million; rape, $101,790; robbery $9.360; assault $10,998; burglary $1,638; larceny $433; auto theft $4, 329. The immediate short-run cost associated with the passage of the shall-issue law is shown in Table 5.

Table 5 about here

The immediate overall cost is $1.2 billion. All crime categories, except rape and burglary, show positive costs due to increases in crime.[16] These laws apparently cause crime to increase in the short run.

However, the results with respect to the coefficients on the post-law trends, presented in Table 6, tell a different story.

Table 6 about here

The number of states with negative post-law trends is greater than the number with positive trends for murder, rape, burglary, and larceny. The US weighted average trend is significantly negative for murder, the most costly crime, and significantly positive only for assault. Because, as time passes, the trend will eventually dominate the shift, the trend is the only coefficient that matters in the long run. Thus, since

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[15] Because Philadelphia was excluded from Pennsylvania's shall issue law until 1995, we treat it as a separate jurisdiction. However, for convenience, we still refer to "states" when counting jurisdictions.

[16] The results are similar if we use only coefficients that are significantly different from zero at the .10 level. In that case the overall net cost to the US is $1.5 billion.

murder is much more costly than assault, this means that the costs of crime will tend to fall in the long run as a result of the shall-issue laws. The implied costs and benefits are presented in Table 7.

Table 7 about here

All crime categories except assault and auto theft show long run benefits from the shall-issue laws. Murder, rape, robbery, and burglary show significant benefits across all states. The overall net benefit to the US is $450 million per year.[17] At this rate, it will take approximately six years for the initial costs to be offset by the eventual long-run benefits. After that, the net benefits increase continuously. This is essentially the same result found by Ayres and Donohue.[18]

Another way to evaluate the effect of these laws is to estimate the overall effect on the states implementing them. We estimate the cumulative effect of the law by combining the estimated coefficient on the dummy variable with the corresponding coefficient on the trend variable using the formula,

$$E_i = \left(N + (N-1)\hat{\gamma} + (N-2)\hat{\gamma}^2 + \ldots + (N-(N-1))\hat{\gamma}^{N-1} \right) \hat{\beta}_{1i} + \left(\sum_{t=0}^{N} t + \hat{\gamma}\left(\sum_{t=0}^{N-1} t \right) + \hat{\gamma}^2 \left(\sum_{t=0}^{N-2} t \right) + \ldots + \hat{\gamma}^{N-1} \right) \hat{\beta}_{2i}$$

where E_i is the effect for state i, N is the number of years the law has been in effect, $\hat{\gamma}$ is the coefficient on the lagged dependent variable, $\hat{\beta}_{1i}$ is the coefficient on the shall-issue dummy for state i, and $\hat{\beta}_{2i}$ is the coefficient on the post-law trend for the same state. This is the cumulative effect over all the years the law has been in existence in each state, up to the year 2000.The net effect for the U.S. as a whole is computed as the population-weighted average. The results are presented in Table 8.

[17] The numbers are very similar using only significant coefficients. In that case the annual net benefit from crime reduction is $398 million per year.

[18] See the appendix for the derivation and proof of the formula used to calculate this result.

Table 8 about here

The number of states experiencing increases in crime is larger than the number with reductions in murder, robbery, assault, and auto theft, confirming the Ayres and Donohue finding for those crimes. On the other hand, there are more negative effects for rape, burglary, and larceny. The results are similar if we only count significant coefficients. However, the overall population-weighted effect for the US is significantly negative for murder and burglary. The only crime for which the net effect of these laws across the US is significantly positive is assault. The other crimes have overall net effects that are not significantly different from zero.

We can estimate the cumulative costs and benefits of the law using the costs of each crime and the cumulative effects from Table 8. The results are presented in Table 9.

Table 9 about here

The number of states with overall increases in the costs of crime is 14 while the number of states with decreases in crime costs is 10. However, the relative costs are very different across states. Louisiana and Tennessee have suffered combined increases in crime costs of approximately $10 billion, while Florida and Georgia have enjoyed benefits of crime reduction of $38 billion. The estimated population-weighted net effect across all states is a reduction in crime costs of $28 billion. The results are similar using only significant coefficients, with an estimated net benefit of $28.4 billion in reduced crime.

The cumulative results are dominated by Florida, which has benefited to the tune of $30.8 billion since passing this law in 1987. Since the net effect across all states is $28 billion, the other states have experienced a net increase in crime amounting to a cost of $2.8 billion. However, this sum is not significantly different from zero, indicating no significant overall costs from the law for other states. Also, even without Florida, there is a long run net benefit of $183 million per year, which is significantly different from zero and which grows continuously. Therefore, even excluding Florida, the state which has apparently benefited most from a right-to-carry law, the overall long run impact of these laws is beneficial.

IV. SUMMARY AND CONCLUSION

We have reviewed the literature on shall-issue laws and find that the clear majority conclude that shall-issue laws reduce crime. However, one recent study by Ayres and Donohue implied an increase in crime.

We extend the data to 2000 and re-analyze the effect of the shall-issue laws using the Ayres and Donohue methodology. We control for the crack epidemic, include dynamic effects, individual state trends, individual county effects, and individual year effects. Like Ayres and Donohue, we find both positive and negative effects among the individual states. We find that shall-issue laws have a significantly negative net effect on murder and burglary across all the adopting states. These laws appear to have a significantly positive effect on assault and no net effect on rape, robbery, larceny, or auto theft. However, in the long run only the trend coefficients matter. We estimate a net benefit of $450 million per year as a result of the passage of these laws. We also estimate that there has been a cumulative overall net benefit of these laws of $28 billion since their passage. We conclude that there is credible statistical evidence that these laws lower the costs of crime.

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APPENDIX

The formula used to calculate the number of years to break-even, assuming that the coefficient on the dummy has a different sign than the coefficient on the trend, is derived as follows.[19] For year 1, the net effect is

$$\Delta y_1 = \beta_d + \beta_{tr}$$

where β_d is the coefficient on the dummy and β_{tr} is the coefficient on the trend. For year 2 the net effect is

$$\Delta y_2 = 2\beta_d + (1+2)\beta_{tr}$$

For year N the net effect is

$$\Delta y_N = N\beta_d + (1+2+...+N)\beta_{tr} = N\beta_d + \left(\sum_{t=1}^{N} t\right)\beta_{tr}$$

The break-even point occurs where

$$\Delta y_N = N\beta_d + \left(\sum_{t=1}^{N} t\right)\beta_{tr} = 0$$

or

$$\beta_d = -\left(\sum_{t=1}^{N} t \Big/ N\right)\beta_{tr}$$

It turns out (see proof below) that

$$\sum_{t=1}^{N} t \Big/ N = (N+1)/2$$

[19] Note that, for calculations where there is a lagged dependent variable, the coefficient on the lagged dependent variable can be ignored here because the coefficient is quite small (the largest coefficient is .37) and appears in both the numerator and denominator.

which implies that

$\beta_d = -\left((N+1)/2\right)\beta_{tr}$

Solving for N yields,

$N = -\left(2\beta_d / \beta_{tr}\right) - 1$.

Also, the same formula applies to the cost-benefit calculations where β_d is the net short- run cost and β_{tr} is the net long run annual benefit.

We now prove the proposition that $\sum_{t=1}^{N} t \Big/ N = \left(N+1\right)/2$. We do this by first proving that $\sum_{t=1}^{N} t = N\left(N+1\right)/2$ by induction. First show that the expression is true for N=1.

Left hand side: $\sum_{t=1}^{1} t = 1$

Right hand side: $N\left(N+1\right)/2 = 1(1+1)/2 = 1$

So the expression is true for N=1.

Now show that the expression is true for N=N+1.

The left hand side is

$$\begin{aligned}
&\sum_{t=1}^{N+1} t = \sum_{t=1}^{N} t + (N+1) \\
&= N(N+1)/2 + (N+1) \\
&= \left[N(N+1) + 2(N+1)\right]/2 \\
&= \left(N^2 + N + 2N + 2\right)/2 \\
&= \left(N^2 + 3N + 2\right)/2
\end{aligned}$$

The right hand side is, for N=N+1,

$$N(N+1)/2 = [(N+1)((N+1)+1)]/2$$
$$= (N+1)(N+2)/2$$
$$= (N^2 + 2N + N + 2)/2$$
$$= (N^2 + 3N + 2)/2$$

So the expression is true for N=N+1. Therefore, the relation is true for all integer values greater than 1; N=1 proves N=2, N=2 proves N=3, etc., assuming that N=1 is true, which was shown in the first step. The last step proves the formula is true for all positive integers.

Since we know that $\sum_{t=1}^{N} t = N(N+1)/2$ it is easy to divide by N to get the result we need:

$$\sum_{t=1}^{N} t \Big/ N = N(N+1)/2N = (N+1)/2 .$$

Table 1
Academic Evidence on the Relationship between Shall-Issue Laws and Crime

Reduce Crime

Refereed journal articles and books

J.R. Lott and D.B. Mustard, Crime, deterrence, and right-to-carry concealed handguns. *Journal of Legal Studies* 26, 1-68 (1997).

______The concealed handgun debate. Journal of Legal Studies 27, 221-243 (1998).

W.A. Bartley and M.A. Cohen, The effect of concealed weapons laws--an extreme bound analysis. *Economic Inquiry* 36, 258-265 (1998).

S.G. Bronars and J.R. Lott, Criminal deterrence, geographic spillovers, and the right to carry concealed handguns. *American Economic Review* 88, 475-479 (1998).

B.L. Benson and B.D. Mast, Privately produced general deterrence. *Journal of Law and Economics* 44, 725-746 (2001).

C.E. Moody, Testing for the effects of concealed weapons laws: Specification errors and robustness. *Journal of Law and Economics* 44,799-813 (2001).

D.B. Mustard, The impact of gun laws on police deaths. *Journal of Law and Economics* 44,635-657 (2001).

D.E. Olsen and M.D. Maltz, Right-to-carry concealed weapons laws and homicide in large U.S. counties: the effect on weapons types, victim characteristics, and victim-offender relationships, *Journal of Law and Economics* 44,747-770 (2001).

F. Plassmann and T. N. Tideman, Does the right to carry concealed handguns deter countable crimes? only a count analysis can say *Journal of Law and Economics*, 44, pp. 771-798 (2001)

J.R. Lott. *More guns, less crime : understanding crime and gun-control laws*. Chicago, University of Chicago Press (1998, 2000).

Non-Refereed

F. Plassmann and J. Whitley, Confirming, ‘more guns, less crime.’ *Stanford Law Review* 54, 1313-1369 (2003).

J. R. Lott and W.M. Landis, Multiple victim public shootings, bombings and right-to-carry concealed handgun laws: contrasting private and public law enforcement. http://papers.ssrn.com/sol3/papers.cfm?abstract_id=161637 (1999, 2001) Published as Chapter 6 of J. R. Lott, *The bias against guns*. Washington, DC, Regnery (2003)

Unpublished

J. R. Lott, Right-to-carry laws and violent crime revisited: clustering, measurement error and state-by-state breakdowns. Working paper, American Enterprise Institute (2004).

Increase Crime

Referred journal articles: none.

Non-Refereed

I. Ayres and J.J. Donohue, Shooting down the more guns, less crime hypothesis. *Stanford Law Review* 54, 1193-1312 (2003).

______The latest misfires in support of the 'more guns, less crime' hypothesis. *Stanford Law Review* 54, 1371-1398 (2003).

J.J. Donohue, The impact of concealed carry laws. In J. Ludwig and P.J. Cook (eds.), *Evaluating Gun Policy*, Washington, DC: The Brookings Institution, 2003, 287-325.

Unpublished: none.

No Significant Effect on Crime

Refereed

D.A. Black and D.S. Nagin, Do right-to-carry laws deter violent crime? *Journal of Legal Studies* 27: 209-219 (1998).

H. Dezhbakhsh and P.H. Rubin,.Lives saved or lives lost--the effects of concealed-handgun laws on crime. *American Economic Review* 88:468-474 (1998).

J. Ludwig, Concealed-gun-carrying laws and violent crime: Evidence from state panel data. *International Review of Law and Economics* 18:239-254 (1998).

M.V. Hood and G.W. Neeley, Packin' in the hood?: examining assumptions of concealed-handgun research. *Social Science Quarterly* 81:523-537 (2000).

G. Duwe, T. Kovandzic, and C.E. Moody, The impact of right-to-carry concealed firearm laws on mass public shootings. *Homicide Studies* 6 (2002)

T. Kovandzic and T.B. Marvell, Right-to-carry concealed handguns and violent crime: crime control through gun decontrol? *Criminology and Public Policy* 2: 363-396 (2003).

E. Hellend and A. Tabarrok, Using placebo laws to test 'more guns, less crime. Advances in Economic Analysis & Policy 4: Article 1 http://www.bepress.com/bejeap/advances/vol4/iss1/art1

National Research Council. *Firearms and Violence: A Critical Review*, Committee to Improve Research Information and Data on Firearms. Charles F. Wellford, John V. Pepper, and Carol V. Petrie, editors, Washington, DC: The National Academies Press (2005).

Kovandzic, Tomislav V., Marvell, Thomas B., and Vieraitis, Lynne E., The Impact of "Shall-Issue" Concealed Handgun Laws on Violent Crime Rates, *Homicide Studies,* 10 (2005).

Non-refereed: none.

Unpublished

G.W. Harrison, D.F. Kennison, and K.E. Macedon, Crime and concealed gun laws: A reconsideration, http://dmsweb.badm.sc.edu/chappell/brownbag/CrimeAndConcealedGunLaws.PDF (2000)

Table 2
Variable names, definitions, and means

ratmur	murder rate per 100,000	5.307
ratrap	rape rate per 100,000	20.637
ratrob	robbery rate per 100,000	45.925
rataga	aggravated assault rate per 100,000	196.571
ratbur	burglary rate per 100,000	758.450
ratlar	larceny rate per 100,000	1777.471
rataut	auto theft rate per 100,000	173.088
shallf	shall-issue dummy	0.278
crack	crack cocaine index	0.878
prison	prison population per capita	0.003
aovio	arrest rate for violent crime	74.247
aopro	arrest rate for property crime	30.366
execrate	execution rate	0.002
unemprt	unemployment rate	6.097
rpcpi	real per capita persional income ($1000)	11.408
rpcui	real per capita unemployment insurance	61.923
rpcim	real per capita income maintenance	182.912
rpcrpo	real per capita retirement payments	1619.632
povrate	poverty rate	14.025
popc	county population	7.895
ppbm1019	percent population black males 10-19	0.008
ppbf1019	percent population black females 10-19	0.008
ppbm2029	percent population black males 20-29	0.007
ppbf2029	percent population black females 20-29	0.008
ppbm3039	percent population black males 30-39	0.007
ppbf3039	percent population black females 30-39	0.007
ppbm4049	percent population black males 40-49	0.005
ppbf4049	percent population black females 40-49	0.006
ppbm5064	percent population black males 50-64	0.006
ppbf5064	percent population black females 50-64	0.007
ppbm65o	percent population black males 65 and over	0.006
ppbf65o	percent population black females 65 and over	0.007
ppwm1019	percent population white males 10-19	0.070
ppwf1019	percent population white females 10-19	0.071
ppwm2029	percent population white males 20-29	0.062
ppwf2029	percent population white females 20-29	0.063
ppwm3039	percent population white males 30-39	0.063
ppwf3039	percent population white females 30-39	0.064
ppwm4049	percent population white males 40-49	0.053
ppwf4049	percent population white females 40-49	0.054
ppwm5064	percent population white males 50-64	0.065
ppwf5064	percent population white females 50-64	0.067
ppwm65o	percent population white males 65 and over	0.063
ppwf65o	percent population white females 65 and over	0.067
ppnm1019	percent population neither males 10-19	0.003
ppnf1019	percent population neither females 10-19	0.003
ppnm2029	percent population neither males 20-29	0.003
ppnf2029	percent population neither females 20-29	0.002
ppnm3039	percent population neither males 30-39	0.003

ppnf3039	percent population neither females 30-39	0.003
ppnm4049	percent population neither males 40-49	0.002
ppnf4049	percent population neither females 40-49	0.002
ppnm5064	percent population neither males 50-64	0.002
ppnf5064	percent population neither females 50-64	0.002
ppnm65o	percent population neither males 65 and over	0.002
ppnf65o	percent population neither females 65 and over	0.002

Table 3
Estimated Coefficients

	murder Coeff	T-ratio	rape Coeff	T-ratio	robbery Coeff	T-ratio	assault Coeff	T-ratio
crack	0.0320	1.45	0.0447	1.57	**0.0709**	3.99	**0.0200**	2.60
prison	**-91.7578**	-3.80	-50.0644	-0.43	**-101.1922**	-5.09	-14.6002	-1.13
aovio	**-0.0004**	-3.06	**-0.0006**	-4.47	**-0.0009**	-5.94	**-0.0008**	-4.85
unemprt	-0.0143	-1.41	**-0.0140**	-1.29	-0.0008	-0.07	-0.0010	-0.21
rpcpi	0.0016	0.29	**-0.0095**	-1.93	0.0025	0.34	-0.0044	-0.88
rpcui	-0.0341	-0.16	-0.0614	-0.17	0.0002	0.68	**-0.2767**	-2.15
rpcim	0.1289	0.44	0.5254	0.88	-0.0001	-0.30	**-0.2309**	-1.70
rpcrpo	-0.0235	-0.21	0.0523	0.25	0.0000	0.48	**0.1293**	1.87
povrate	-0.0005	-0.10	0.0085	0.90	0.0020	0.52	-0.0003	-0.14
popc	**0.0006**	1.65	**-0.0040**	-4.77	**-0.0005**	-1.98	0.0001	0.25
Y(t-1)	0.0130	1.51	**0.1241**	4.24	**0.1104**	6.38	**0.3663**	13.81
R-square	0.65		0.66		0.85		0.83	
N	54169		54148		58844		58830	

	burglary Coeff	T-ratio	larceny Coeff	T-ratio	auto Coeff	T-ratio
crack	**0.0284**	3.56	**0.0284**	3.56	**0.0634**	3.90
prison	**-38.9346**	-2.61	**-38.9346**	-2.61	**-85.4660**	-5.72
aovio	**-0.0005**	-5.95	**-0.0005**	-5.95	**-0.0006**	-5.10
unemprt	0.0077	1.46	0.0077	1.46	-0.0037	-0.41
rpcpi	**-0.0078**	-2.91	**-0.0078**	-2.91	**0.0105**	1.87
rpcui	0.0561	0.59	0.0561	0.59	0.3479	1.07
rpcim	-0.0169	-0.15	-0.0169	-0.15	-0.1613	-0.39
rpcrpo	0.0834	1.56	0.0834	1.56	0.0853	0.87
povrate	0.0006	0.29	0.0006	0.29	0.0015	0.56
popc	**-0.0006**	-3.29	**-0.0006**	-3.29	**-0.0011**	-1.80
Y(t-1)	**0.3656**	6.03	**0.3656**	6.03	**0.2788**	4.81
R-square	0.86		0.87		0.83	
N	61550		61550		61551	

Notes: the dependent variable is the crime rate logged. Because of the relatively large number of zeroes in the murder and rape counts we added .10 to the per capita rates before taking logs. Y(t-1) is the lagged dependent variable. Coefficients in bold are significantly different from zero at the .10 level, two-tailed. We suppress the estimated coefficients on the 36 demographic variables, the year dummies, the individual state trends, and the 24 shall issue dummies and post-law trends. Complete results are available from http://cemood.people.wm.edu/whither.zip. The execution rate was not significant in the murder equation and was dropped. The overall results were unchanged.

Table 4
Shall-issue dummy coefficients

	Murder Coeff	T-ratio	Rape Coeff	T-ratio	Robbery Coeff	T-ratio	Assault Coeff	T-ratio	Burglary Coeff	T-ratio	Larceny Coeff	T-ratio	Auto Theft Coeff	T-ratio
AK	0.125	1.38	**-0.517**	-4.31	-0.024	-0.31	0.038	0.89	-0.021	-0.36	-0.044	-0.86	**-0.133**	-2.38
AZ	**0.264**	6.2	-0.064	-0.82	**0.171**	4.70	**0.053**	1.97	**0.073**	3.71	**0.058**	2.69	**0.175**	3.72
AR	0.048	1.04	0.031	0.31	**-0.073**	-2.19	**0.099**	4.56	**-0.075**	-4.27	0.004	0.24	-0.023	-0.72
FL	-0.089	-1.22	**-0.181**	-2.87	**0.141**	1.88	**0.073**	2.82	0.005	0.18	-0.014	-0.58	**0.154**	2.71
GA	**-0.200**	-4.28	-0.052	-0.89	**-0.151**	-3.62	**-0.052**	-2.67	**-0.124**	-5.11	**-0.081**	-4.65	**-0.167**	-4.74
ID	**0.978**	23.04	**0.302**	2.24	0.093	1.20	0.030	0.99	-0.015	-0.44	**0.070**	2.04	0.094	1.59
KY	0.046	0.90	**-0.301**	-3.98	**0.277**	5.29	**0.160**	6.36	-0.025	-1.03	**-0.079**	-4.06	**0.075**	1.70
LA	**0.381**	6.15	0.113	1.62	**0.287**	4.44	0.056	1.58	0.043	1.59	**0.052**	2.32	**0.235**	5.49
ME	**0.460**	9.70	0.121	1.64	**-0.144**	-3.08	**-0.151**	-5.15	0.007	0.32	**0.051**	3.01	0.077	1.61
MS	0.067	1.21	-0.034	-0.47	**0.143**	3.38	**0.115**	4.28	-0.031	-0.97	0.006	0.28	-0.044	-1.24
MT	0.008	0.22	0.233	1.56	**-0.430**	-6.90	**-0.210**	-7.92	**0.117**	3.79	0.029	1.01	0.023	0.43
NV	**0.551**	12.65	**0.151**	1.99	**0.107**	2.48	**0.174**	5.24	**0.159**	5.10	**0.107**	3.20	**0.142**	4.79
NC	0.009	0.20	0.053	0.41	**0.090**	2.11	**0.102**	3.61	-0.026	-0.83	**0.061**	2.29	**0.201**	5.66
OK	**0.090**	2.21	0.060	1.14	-0.062	-1.33	0.006	0.27	0.013	0.46	0.041	1.39	-0.029	-0.69
OR	**-0.213**	-5.18	0.025	0.30	**-0.240**	-4.14	**0.049**	1.91	**-0.084**	-2.73	0.016	0.64	-0.016	-0.31
PA	-0.022	-0.51	0.064	1.19	**-0.061**	-1.87	**-0.051**	-2.27	-0.021	-1.06	0.012	1.15	0.004	0.17
PH	-0.024	-0.63	**-0.344**	-5.41	**-0.060**	-1.77	**-0.213**	-7.70	**-0.107**	-2.76	**-0.235**	-8.04	-0.047	-1.18
SC	0.050	1.05	-0.126	-1.54	-0.052	-0.98	0.055	1.60	**-0.052**	-1.90	-0.025	-1.14	0.074	1.60
TN	-0.026	-0.69	**-0.154**	-2.51	**-0.091**	-2.85	0.046	1.48	**-0.036**	-1.68	**-0.037**	-1.71	-0.047	-1.41
TX	-0.055	-1.16	0.103	0.44	0.046	0.85	0.024	0.88	**0.073**	2.12	0.050	1.49	0.078	1.59
UT	**0.100**	1.66	-0.034	-0.38	**0.078**	1.74	**0.214**	6.38	**0.079**	1.96	-0.052	-1.58	**0.188**	4.55
VA	0.030	0.60	**0.107**	1.97	-0.054	-1.39	**-0.040**	-2.09	**-0.072**	-2.88	-0.012	-0.52	**-0.101**	-2.47
WV	**0.285**	6.44	0.100	1.47	-0.064	-1.51	**-0.075**	-2.70	**0.063**	2.06	**0.078**	4.26	**-0.093**	-2.12
WY	**-0.266**	-3.92	-0.003	-0.02	**0.512**	7.69	-0.042	-1.18	**0.145**	5.18	**0.071**	2.31	**0.165**	2.91
US	0.006	0.06	-0.007	0.01	0.008	0.10	**0.031**	6.36	-0.010	0.30	0.009	0.39	**0.050**	6.47
negative	8		11		13		8		13		9		10	
significant	3		5		8		7		8		4		4	
positive	16		13		11		16		11		15		14	
significant	8		3		9		9		7		8		8	

Notes: coefficients in bold are significant at the .10 level. The test statistics for the US weighted average are F-ratios corresponding to the null hypothesis that the weighted average is zero. PH is Philadelphia.

Table 5
Short run costs and benefits
Millions of 2000 dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	11.69	-13.68	-0.18	0.93	-0.15	-0.34	-1.59	-3.32
AZ	383.40	-9.29	10.40	11.02	5.73	3.67	29.71	434.65
AR	42.60	2.93	-2.12	9.84	-3.21	-0.17	-1.12	48.75
FL	-419.15	-111.36	56.84	55.88	9.05	0.69	59.40	-348.64
GA	-551.77	-16.46	-24.31	-14.03	-18.74	-6.43	-29.36	-661.10
ID	92.12	8.41	0.13	0.72	-0.41	0.66	0.50	102.12
KY	15.44	-13.95	8.10	10.09	-1.12	-1.60	1.28	18.25
LA	977.93	19.70	31.62	14.60	4.09	3.74	26.83	1078.52
ME	44.72	2.06	-0.38	-2.34	0.37	0.80	0.80	46.02
MS	43.52	-2.49	2.31	4.22	-2.48	-0.36	-0.92	43.79
MT	0.37	2.68	-0.40	-1.21	0.48	0.08	-0.21	1.79
NV	306.86	14.27	4.96	15.20	4.88	2.23	6.34	354.75
NC	21.77	12.42	10.72	32.54	-4.47	5.93	19.16	98.07
OK	122.97	8.92	-2.18	1.08	0.16	1.25	-2.88	129.30
OR	-79.23	3.37	-9.19	4.60	-5.31	0.10	-1.78	-87.44
PA	-21.16	14.02	-4.43	-9.42	-1.39	1.45	1.02	-19.92
PH	-35.87	-27.03	-7.65	-15.82	-2.37	-4.45	-2.34	-95.52
SC	57.22	-24.20	-3.16	16.57	-3.94	-1.44	5.12	46.16
TN	-39.44	-36.50	-8.86	10.85	-5.23	-2.25	-5.08	-86.50
TX	-321.52	89.52	14.46	19.90	18.25	8.78	29.22	-141.38
UT	26.31	-2.85	0.94	9.20	1.80	-2.43	5.62	38.59
VA	46.15	16.93	-3.22	-3.66	0.77	4.32	-2.08	59.20
WV	118.94	3.53	-0.47	-1.15	1.25	0.97	-1.24	121.83
WY	-15.71	-0.04	0.38	-0.46	0.64	0.35	0.48	-14.35
US	**828.17**	**-59.08**	**74.29**	**169.16**	-1.33	**15.53**	**136.88**	**1163.63**

Notes: bold indicates that the sum across states is significantly different from zero at the .10 level.
PH is Philadelphia

Table 6
Shall-issue post-law trend coefficients

	Murder		Rape		Robbery		Assault		Burglary		Larceny		Auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	**-0.104**	-4.49	-0.041	-0.42	**-0.093**	**-4.58**	-0.023	-1.49	**-0.030**	-1.95	**-0.030**	-2.06	**-0.046**	-2.34
AZ	**-0.055**	-2.69	0.026	0.91	-0.003	-0.15	-0.010	-1.02	-0.003	-0.34	**-0.024**	-2.87	**-0.060**	-4.38
AR	**-0.108**	-6.22	**-0.081**	-2.98	0.004	0.17	**0.048**	5.21	-0.013	-1.06	-0.005	-0.51	-0.004	-0.20
FL	**-0.054**	-4.24	**0.032**	1.76	**-0.085**	-6.59	0.003	0.42	**-0.031**	-4.43	-0.003	-0.45	-0.007	-0.49
GA	0.010	1.29	**-0.066**	-5.39	-0.016	-1.26	-0.005	-0.80	**-0.016**	-2.66	-0.007	-0.96	**0.027**	1.95
ID	**-0.057**	-4.55	-0.003	-0.15	**0.077**	3.77	**0.032**	4.06	0.004	0.50	**-0.021**	-2.62	0.016	0.95
KY	-0.025	-1.16	**-0.103**	-4.81	**-0.050**	-2.17	**-0.081**	-6.86	**-0.025**	-1.66	-0.007	-0.61	-0.016	-1.08
LA	0.002	0.09	0.038	1.02	0.039	1.58	0.008	0.47	**0.033**	2.32	**0.024**	2.21	**0.059**	2.07
ME	**0.025**	2.95	-0.016	-0.71	-0.013	-1.16	**0.014**	2.35	-0.004	-0.60	-0.001	-0.12	-0.003	-0.37
MS	**0.053**	4.75	**0.059**	2.97	**0.084**	5.50	**0.067**	6.93	**0.049**	7.05	**0.058**	8.50	**0.100**	6.46
MT	**-0.025**	-1.84	-0.030	-1.51	**0.131**	13.39	**0.202**	24.85	-0.002	-0.37	**0.017**	2.92	0.007	0.89
NV	**-0.131**	-7.66	**-0.077**	-1.84	-0.023	-1.22	**-0.062**	-4.89	**-0.024**	-2.52	**-0.048**	-4.73	-0.010	-0.57
NC	-0.010	-0.57	**-0.083**	-1.65	-0.003	-0.15	-0.015	-1.34	-0.015	-1.13	-0.012	-1.04	0.020	0.93
OK	-0.002	-0.13	**-0.041**	-1.75	0.003	0.15	-0.002	-0.26	-0.010	-0.97	-0.010	-0.88	0.002	0.11
OR	**-0.083**	-8.24	-0.038	-1.64	**-0.038**	-3.43	**0.046**	6.33	**-0.013**	-1.78	0.004	0.67	**-0.022**	-2.17
PA	0.008	1.21	**-0.026**	-2.25	**0.030**	4.16	**0.011**	2.55	0.000	0.03	**0.014**	3.54	-0.006	-1.08
PH	-0.003	-0.28	**0.062**	2.50	0.032	1.18	**0.050**	4.27	-0.012	-1.64	0.007	0.76	0.035	1.14
SC	0.004	0.15	-0.069	-1.48	0.018	0.61	-0.020	-1.01	-0.022	-1.19	-0.006	-0.34	0.042	1.33
TN	**0.113**	9.97	**0.086**	4.74	**0.116**	6.81	**0.072**	8.03	**0.054**	5.96	**0.061**	7.44	**0.077**	4.47
TX	0.000	-0.01	-0.043	-1.61	-0.007	-0.45	**-0.016**	-2.26	0.005	0.52	-0.003	-0.28	-0.004	-0.25
UT	-0.016	-0.78	0.004	0.10	0.038	0.79	0.012	0.88	0.007	0.36	-0.009	-0.40	0.014	0.34
VA	0.001	0.09	0.004	0.42	**0.056**	3.34	**0.027**	3.81	0.002	0.36	0.004	0.60	0.025	1.52
WV	**-0.098**	-9.21	**-0.046**	-2.46	0.001	0.03	**0.105**	11.34	0.003	0.34	0.000	-0.05	**0.018**	1.71
WY	**0.167**	7.53	-0.020	-0.81	0.018	0.77	**0.059**	5.78	**-0.021**	-2.12	**-0.018**	-1.75	0.008	0.47
US	**-0.017**	4.74	-0.022	1.77	0.0003	0.00	**0.011**	3.19	-0.004	0.02	0.003	0.02	0.008	0.35
negative	15		16		10		9		15		16		10	
significant	8		8		4		3		6		5		3	
positive	9		8		14		15		9		8		14	
significant	4		4		6		12		3		5		5	

Notes: coefficients in bold are significant at the .10 level. The test statistics for the coefficients for the individual states are t-ratios. The test statistic for the US weighted average is the F-ratio corresponding to the null hypothesis that the weighted average is zero. PH is Philadelphia.

Table 7
Long run costs and benefits
Millions of 2000 dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-9.70	-1.08	-0.72	-0.56	-0.16	-0.23	-0.55	-13.00
AZ	-80.37	3.81	-0.19	-2.03	-0.29	-1.97	-11.18	-92.22
AR	-96.03	-7.66	0.12	4.79	-0.52	-0.16	-0.14	-99.60
FL	-254.54	19.39	-34.08	2.11	-13.80	-0.50	-2.54	-283.96
GA	27.76	-20.62	-2.52	-1.34	-2.92	-0.72	4.81	4.47
ID	-5.32	-0.09	0.11	0.77	0.05	-0.26	0.12	-4.63
KY	-8.43	-4.76	-1.47	-5.10	-0.54	-0.11	-0.43	-20.83
LA	6.29	6.72	4.28	2.16	2.92	1.70	6.84	30.91
ME	2.47	-0.27	-0.03	0.21	-0.06	-0.01	-0.03	2.28
MS	34.48	4.28	1.37	2.47	1.81	1.02	1.79	47.21
MT	-1.14	-0.34	0.12	1.17	-0.01	0.11	0.04	-0.05
NV	-72.85	-7.27	-1.06	-5.46	-0.80	-1.11	-0.49	-89.03
NC	-22.43	-19.46	-0.37	-4.73	-2.42	-1.16	1.92	-48.66
OK	-2.14	-6.11	0.12	-0.40	-0.71	-0.43	0.13	-9.54
OR	-30.93	-5.17	-1.45	4.27	-0.67	0.16	-1.23	-35.03
PA	7.24	-5.67	2.20	2.10	0.01	0.97	-0.87	5.99
PH	-4.91	4.84	4.13	3.68	-0.32	0.13	3.65	11.20
SC	4.58	-13.20	1.10	-5.93	-1.75	-0.33	2.88	-12.64
TN	170.38	20.46	11.32	17.02	4.61	3.27	9.04	236.10
TX	-0.92	-37.90	-2.12	-13.74	1.64	-0.71	-1.78	-55.53
UT	-4.32	0.35	0.46	0.52	0.18	-0.33	0.45	-2.68
VA	1.06	0.69	3.35	2.48	0.13	0.25	1.90	9.86
WV	-41.03	-1.63	0.00	1.62	0.05	0.00	0.22	-40.76
WY	9.86	-0.33	0.01	0.64	-0.11	-0.12	0.03	9.98
US	**-370.94**	**-71.03**	**-15.33**	6.74	**-13.67**	-0.53	**14.61**	**-450.15**

Notes: bold indicates that the sum across states is significantly different from zero at the .10 level.
PH is Philadelphia

Table 8
Cumulative effect of the shall-issue laws on crime

	Year Passed	N	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto
AK	1994	6	**-1.430**	**-3.957**	**-2.087**	-0.252	**-0.808**	**-0.894**	**-1.771**
AZ	1994	6	0.422	0.169	**0.960**	0.112	0.293	-0.241	-0.304
AR	1995	5	**-1.376**	-1.067	-0.302	**1.221**	**-0.590**	-0.107	-0.220
FL	1987	13	**-6.064**	0.516	**-5.877**	**1.197**	**-2.563**	-0.190	1.538
GA	1989	11	**-1.532**	**-4.910**	**-2.685**	**-0.905**	**-2.244**	**-1.088**	-0.030
ID	1990	10	**6.257**	**2.570**	**4.314**	**1.696**	-0.105	**-0.462**	**1.367**
KY	1996	4	-0.068	**-2.232**	**0.606**	-0.169	**-0.460**	**-0.496**	0.032
LA	1996	4	**1.550**	**0.834**	**1.539**	**0.306**	**0.515**	**0.444**	**1.518**
ME	1985	15	**9.961**	-0.110	**-3.736**	-0.635	-0.104	0.904	1.054
MS	1990	10	**3.616**	**2.897**	**6.063**	**4.863**	**2.026**	**2.996**	**5.006**
MT	1991	9	**-1.034**	0.761	**2.018**	**7.201**	**0.774**	**0.869**	-0.041
NV	1995	5	**0.792**	-0.399	0.192	-0.066	**0.378**	**-0.237**	**0.495**
NC	1995	5	-0.099	**-0.981**	0.402	**0.288**	**-0.355**	0.121	**1.299**
OK	1995	5	**0.425**	-0.316	-0.259	-0.003	-0.145	-0.004	-0.176
OR	1990	10	**-6.700**	**-1.852**	**-4.482**	**3.022**	**-1.704**	0.225	**-1.518**
PA	1989	11	0.257	-1.006	**1.336**	0.189	-0.139	**1.127**	-0.340
PH	1995	5	-0.170	**-0.795**	0.186	**-0.322**	**-0.632**	**-1.009**	0.418
SC	1996	4	0.238	**-1.196**	-0.028	0.023	**-0.426**	-0.161	**0.715**
TN	1994	6	**2.219**	**0.889**	**1.899**	**1.776**	**0.767**	**1.025**	**1.358**
TX	1995	5	-0.278	-0.139	0.129	-0.126	**0.349**	0.122	0.263
UT	1995	5	0.255	-0.108	0.957	**1.253**	**0.460**	**-0.453**	**1.080**
VA	1988	12	0.407	**1.630**	**3.737**	**1.634**	0.255	**1.028**	1.629
WV	1989	11	**-3.352**	-1.936	-0.667	**6.119**	0.913	0.897	0.090
WY	1994	6	**1.912**	-0.440	**3.449**	**0.979**	0.326	-0.054	**1.032**
US			**-1.169**	-0.589	-0.571	**0.971**	**-0.522**	0.262	0.549
Negative			11	16	9	8	13	13	8
Significant			7	7	5	2	9	7	2
Positive			13	8	15	16	11	11	16
Significant			8	5	10	13	7	6	9

Notes: coefficients in bold are significant at the .10 level using standard F-tests. Coefficients are the estimated percentage change in crime over the N years the law has been in effect. PH is Philadelphia.

Table 9
Cumulated costs and benefits of the shall-issue law
Millions of 2000 dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-134	-116	-17	-7	-6	-9	-26	-315
AZ	626	24	64	40	42	-21	-48	727
AR	-1238	-109	-10	163	-33	-4	-10	-1241
FL	-28938	339	-2602	1375	-1691	-53	736	-30834
GA	-4289	-1722	-480	-357	-601	-174	-19	-7641
ID	597	80	7	58	-2	-7	13	745
KY	-23	-113	20	-10	-13	-10	1	-147
LA	4013	159	184	106	60	42	216	4781
ME	979	-2	-11	-16	-2	17	12	977
MS	2357	234	108	256	107	74	115	3251
MT	-48	10	2	58	6	8	0	35
NV	448	-40	10	-1	19	-6	32	462
NC	-229	-249	53	134	-80	19	158	-195
OK	589	-50	-10	0	-13	0	-16	499
OR	-2521	-278	-190	402	-135	14	-111	-2819
PA	247	-243	106	44	-22	116	-59	188
PH	-255	-71	25	-38	-23	-28	52	-340
SC	277	-249	-2	15	-44	-12	60	45
TN	3363	217	194	548	79	68	186	4656
TX	-1638	-123	45	-131	163	47	154	-1482
UT	68	-10	13	74	16	-23	44	182
VA	644	289	245	214	30	106	160	1688
WV	-1414	-76	-5	134	27	15	1	-1318
WY	113	-8	3	13	3	0	4	128
US	**-26406**	**-2105**	**-2250**	**3075**	**-2114**	**177**	**1655**	**-27969**

Notes: bold indicates that the sum across states is significantly different from zero at the .10 level.
PH is Philadelphia